and various sore trials, was at last injured by the pestilential miasmatic emanations of the mangrove swamps of Engano, and he came away just in time to save his life. At the end of July he embarked at Batavia, and by the end of August we had the pleasure of welcoming him back to Florence.

He has already given brief accounts of his exploration of the Toba country in letters and in two lectures recently delivered at Rome and Florence. He is now busy working out his ethnological materials; those pertaining to zoology have already been partly examined by specialists. The principal novelties in zoology are, as was to be expected, from Engano. Amongst those already published I recall the following:—Birds: *Graucalus enganensis*, *Pericrocotus modiglianii*, *Zosterops incerta*, *Geocichla leucolæma*, *Calornis enganensis*, *Gracula enganensis*, *Carpophaga œnothorax*, and *Macropygia cinnamomea*, are new species from Engano, recently described by Count Salvadori; besides, Modigliani has established the hitherto unknown habitat of a lovely Parroquet (*Palæornis modesta*). Reptiles: *Draco modiglianii*, *Lycosoma relictum*, and *Coluber enganensis* are new species recently described by Dr. Vinciguerra, also from Engano.

On the anthropological and ethnological materials collected by Dr. Modigliani on this voyage, which are many and of great interest, I intend writing a special report, which will be published elsewhere. I will therefore, in concluding this communication, merely draw attention to their scientific value, mentioning the more important series.

Amongst the Toba Battaks, Modigliani was not successful in obtaining human skulls, but, with the help of *guru* Samalain, he was able to take with rare ability and perfection a magnificent series of plaster masks of the face from life, both of men and women. He has made, besides, a splendid collection of photographs illustrating the people and their habits. A most extensive and perfect series of actual specimens and carefully constructed models made on the spot illustrate completely the houses, *sopos*, and boats, with their ornaments; the agricultural implements, house furniture, dress and personal ornaments; food and stimulants, with the utensils pertaining thereto; work-tools, and weapons offensive and defensive; religion and superstitions, witchcraft and literature. The carvings on the houses, and the patterns of the textile fabrics of the Toba Battaks, are indeed remarkable as specimens of the finest style of ornamentation. Amongst the numerous series illustrating the crude religion and manifold superstitions of this singular people, I may mention the carved wooden figures, with movable arms and a square hole in the chest, in which is the sacred relic (*pangulubalang*) or talisman containing part of the remains of a sacrificed child; these figures, of which Modigliani has collected quite a number, are as rudely made as the *karwars* of North-west New Guinea. Of the remarkable magic staffs, called generically *tungal pagñaluan*, but which appear to have individual names, Modigliani has secured seven; they consist of superposed figures, more or less conventionalized, but beautifully carved in a hard dark wood in most cases, in which the human figure and those of the elephant, buffalo, lizard, and serpent are variously entwined. Modigliani thinks that each of these staffs symbolizes the history of the village or clan to which it belongs, in which case they might be compared to the genealogical Maori staffs, like the one recording the history of the Ngatirangi tribe, now in the British Museum. As samples of the little-known literature of the Battaks, Modigliani was fortunate enough to secure twenty of their books, now rare. They are ancient-looking tomes of various sizes, bound in wooden boards; the leaves are of beaten bark, the writing minute, mystic figures (*hatiha*) being occasionally intercalated. These books, written by learned *gurus*, are of a sacred, medicinal, and encyclopædic nature, and much very valuable information on the Battaks will certainly be gained by their translation.

At Engano, Modigliani was able to obtain three skulls, and took six excellent plaster casts from the living. His series of photographs is also fine; but unfortunately some of the best were spoiled by the heat. I have already noted the remarkable resemblance which the natives of Engano show with those of the Nicobar Islands. There are amongst them faces which also recall Polynesian and especially Micronesian types. Thus the photographs taken by my friend, over a hundred, are of great ethnological value. The collections illustrating the ethnography of the Engano islanders are, I should say, complete: besides beautiful models of the singular houses and canoes, and actual specimens of the ornamented portions, viz. doors, cross-seats, &c., the mourning and ceremonial dresses and ornaments, house utensils, weapons, &c., are represented by a great number of carefully selected specimens.

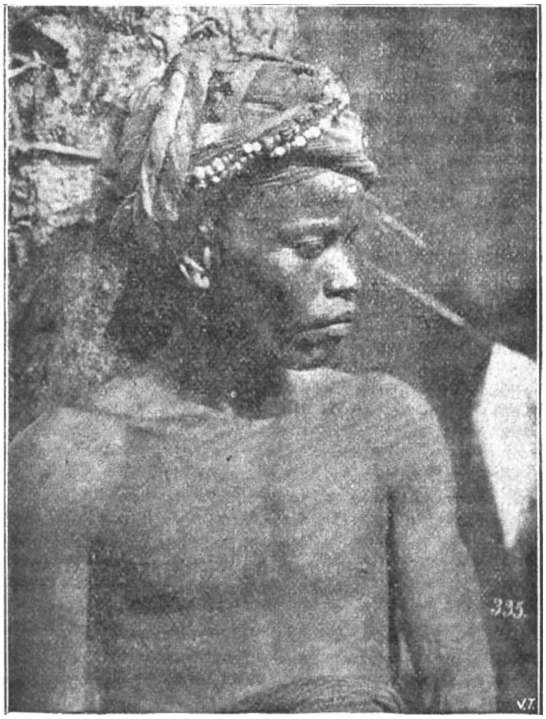

FIG. 4.—Battak gentleman.

In conclusion, I can only say that Dr. Modigliani has done much excellent work, and that we may look forward with pleasure to the publication of the results of his investigations, both in zoology and in anthropology.

HENRY H. GIGLIOLI.

A MODERN REVIVAL OF PROUTS HYPOTHESIS.[1]

IT frequently happens in the history of science that the line of thought engendered by one branch of study proves applicable in a totally distinct field. In accordance with this principle a great stimulus is occasionally given in some particular line of research by the encroachment of an investigator who brings the vitalizing ideas derived from his own work to bear upon a new subject. It was with some such notions as these that we were predisposed to welcome Mr. Wilde's attempt to deal with the greatest of all the problems presented by modern chemistry, but a careful consideration of the author's views has, we regret to say, left us in a state of disappointment for reasons which we will endeavour to explain to the readers of NATURE.

---

[1] "On the Origin of Elementary Substances and on some New Relations of their Atomic Weights." By Henry Wilde, F.R.S. (London: Kegan Paul, Trench, Trübner, and Co., 1892.)

The work under consideration is a quarto pamphlet of eighteen pages and a folding table giving the author's and other arrangements of the chemical elements. It consists of a preface dated May 1892, and a paper reprinted with additional notes from the Memoirs of the Manchester Literary and Philosophical Society for 1883 and 1887, the results having been first made known in the Proceedings of the same Society for April, 1878. The preface and paper are followed by translations of the same into French. Everything emanating from a recognized authority in a distinct department of science is worthy of consideration by chemists, and there are scattered throughout the work many statements which we cannot but endorse. There are, moreover, a few suggestions here and there which might be fruitful, and although the general result is disappointing, it is opportune that the author should have restated his hypothesis at a time when all chemists have more or less assimilated the views of Newlands, Mendeléef, Lothar Meyer, and their followers. It may be stated at the outset that Mr. Wilde's theory has nothing to do with an electrical origin of the elements as his reputation as an electrician might at first lead us to imagine.

In very brief terms the author's theory is that the elements have been evolved from hydrogen by a process of nebular condensation. In so far as he regards the elements as polymerides (as we might now express it) of hydrogen, there is nothing new in the idea. It is Prout's hypothesis pure and simple. We are far from asserting that this hypothesis has been disproved; there is a fascinating simplicity about it—it is so much in harmony with the general course of nature that matter should have been evolved from some primordial stuff that we should like it to be true; but unfortunately the most exact determinations of atomic weights have in later times not always conformed to the requirements of the hypothesis. Mr. Wilde in effect, if not in words, says *tant pis pour les faits!* These numbers ought to be whole multiples of the atomic weight of hydrogen, and Mr. Wilde unhesitatingly makes them so. In some cases the discrepancy between the observed atomic weights and those calculated from the theory is so great—apart from the doubling or other manipulation of some of the old numbers—that this alone will damage his case in the eyes of those who know the scrupulous care taken and the variety of methods resorted to in order to secure purity of material in such determinations. We give a few examples:—

	Accepted Atomic Weight.	Calculated.		Accepted Atomic Weight.	Calculated.
Cu	63·18	62	Cr	52·45	54
Be	9·08	8	W	183·6	186
Sc	43·97	42	Si	28·2	35
Ga	69·9	96	Ni	58·6	56
Y	88·9	123	Co	58·6	56
In	113·6	150	Ir	192·5	196
Ta	182	185	Os	191·12	196

A large number of atomic weights not given above differ by one unit from the experimental results; in fact, more than half the existing determinations are in the light of the present theory erroneous to a most humiliating extent.

We are not bigoted in our faith respecting the unassailable accuracy of the determinations of these constants; we all know the enormous difficulties which meet the chemist in his attempts to obtain his compounds in a state of purity. In one part of his paper the author suggests "that slight differences in the determinations may arise from the latent affinity which some elements have for minute quantities of another," which is a reasonable supposition in its way, although not very happily expressed. But later he somewhat inconsistently remarks "that these discrepancies are due to . . . some unknown cause which prevents their [*i.e.* Cu, Zn, &c.] true atomic weights from being ascertained."

From a purely philosophical standpoint the author's proposed emendations of the atomic weights are perfectly legitimate. If it can be satisfactorily proved that these constants are the numerical consequences of some general law requiring that the relative combining weights referred to hydrogen should be whole numbers, it is correct to conclude that our determinations are, through experimental error, difficulty of separation, &c., faulty. That some such law exists has been surmised again and again, but unfortunately the proof has not yet been found. Now the central idea of Mr. Wilde's paper is that there is an analogy between Bode's law of the planetary distances and the numerical relationships between the atomic weights, and he even attempts to show that this analogy is the result of a causal connection between the phenomena. This is the most important suggestion in the work, as the whole novelty centres in this idea, and the subsequent acceptance of his views will turn upon the strength of his case in demonstrating these two points: first, that Bode's "law" is the expression of a physical reality; and, secondly, that the numerical relations between the atomic weights are the physical expressions of a causal connection between the distances of the planets and the condensation of the primordial matter (? hydrogen.)

The first point is purely astronomical, and we prefer to let astronomers speak on the subject. Prof. Simon Newcomb says ("Popular Astronomy"):—

> It is true that many ingenious people employ themselves from time to time in working out numerical relations between the distances of the planets, their masses, their times of rotation, and so on, and will probably continue to do so; because the number of such relations which can be made to come somewhere near to exact numbers is very great. This, however, does not indicate any law of nature. If we take forty or fifty numbers of any kind—say the years in which a few persons were born; their ages in years, months, and days at some particular event in their lives; the numbers of the houses in which they lived; and so on—we should find as many curious relations among the numbers as have ever been found among those of the planetary system.

The author thus gets but little support from astronomy and it is to be observed that in the list of planetary distances which he gives he stops short at Uranus; Neptune occupies an awkward position for Bode's "law." The flight which is taken in connecting this "law" with the atomic weights is, however, a bold one and worthy of being given in the author's own words. After stating the nebular hypothesis he says:—

> That this gaseous or primordial substance consisted of a chaotic mixture of the sixty-five elements known to chemists is a notion too absurd to be entertained by any one possessing the faculty of philosophic thinking, as the regular gradation of properties observable in certain series of elements clearly shows that elementary species are not eternal, but have a history, which it is the proper object of physical science to unfold.

With this we most cordially agree, and as the same idea has been repeatedly expressed by chemists and physicists, we do not imagine that it is likely to be controverted. Then he continues:—

> One of the principal facts which, to my mind, establishes the nebular theory of the formation of planetary systems on a firm basis is Bode's empirical law of the distances of the members of the solar system from each other and from the central body, as in this law is comprehended the idea of nebular condensation in definite proportions. Now, if elementary species were created from a homogeneous substance possessing a

capacity for change in definite proportions, it is probable that the greater number of elements would be formed during or after the transition of the nebular matter from the annular to the spheroidal form. Moreover, as great cosmic transitions are not made *per saltum*, it might be expected that some modification of the law of nebular condensation into planetary systems, as exhibited in Bode's law, would be found on the further condensation of the primitive matter into elementary species.

There appears to be a flaw in this chain of reasoning which weakens the whole paragraph. It is difficult to see how a law, which the author himself describes as "empirical," can establish a theory on a "firm basis." We admit that an empirical law may be of use—Bode's law is a case in point—but surely it must pass beyond the stage of empiricism before it can establish anything on a firm basis. The astronomical foundation having therefore been shown to be insecure, or, in the opinion of astronomers, even non-existent, it remains next to consider the second point, with respect to which we shall let the author speak for himself:—

> One objection raised against the theory which I have propounded on the origin and compound nature of the elements I will remark upon, is an alleged want of causal connection between the series of planetary distances and a series of atomic weights. Now, considering that specific gravities and atomic weights are admittedly correlated properties of the elements, and that specific gravities are fundamentally correlated with the dimensional properties of space, it follows that planetary condensations within interplanetary space are correlated directly with atomic condensations and atomic weights within that space. Hence the law that every increase of atomic weight, in a well-defined odd or even series of elements, is attended by an increase of specific gravity, is a natural consequence of the theory.

This is quoted from the preface; the mechanism of the process is described in the paper as follows:—

> In the present hypothesis it is assumed:—(1) That a mass of hydrogen, of a curvilinear form, acquired a motion of rotation about a central point, which caused it to take a spiral or convolute form. (2) As each successive spiral or convolution was formed, the particles of hydrogen combined with themselves, as far as the septenary combination, to constitute the type of each series of elements—the number of types or series being equal to the number of convolutions of the rotating gas. According to this view, the elementary groups may be represented as forms of Hn, H2n, H3n, H4n, H5n, H6n, H7n; the internal convolutions forming the highest type, H7n, and the outer convolution the type Hn. (3) That on a further condensation of the elementary matter a transition from the spiral to the annular form occurred, during or after which the series under each type was generated in concentric zones and in the order of their atomic weights, until the highest member of each species was formed. (4) That as the elementary vapours begin to condense, or assume the liquid form, their regular stratification would be disturbed by eruptions of the imprisoned vapours from the interior of the rotating mass. The disturbance would be further augmented by the subsequent combination of the negative with the positive elements, and also by the various solubilities of their newly-formed compounds; so that the evidence of such stratification of the elementary vapours as I have indicated must necessarily be more fragmentary than that of the geological record.

In support of this last statement the author mentions the well-known association of allied elements in minerals.

The idea of an evolution of matter by a process of nebular condensation as above set forth is to be found under various forms in the writings of Herbert Spencer, of Sterry Hunt, Lockyer, and others. Also, it may be remarked in passing, that the hypothesis of stratification in the order of density was applied to the sun by Johnstone Stoney about the year 1867. In fact the general notion of elementary evolution is so obvious that it cannot fail to present itself again and again to those who think over such problems as are here dealt with. For the sake of chemical philosophy we only wish that this speculation could be placed on a firmer basis of observation or experiment—if for no other reason in order that the minds of chemists might be cleared of this inorganic *Urschleim* in which since the time of Prout they have been compelled to wallow.

Reduced to its ultimate terms it will appear, then, that Mr. Wilde's view is a combination of the nebular with Prout's hypothesis, the latter being stated with a precision and boldness which certainly goes beyond any utterance on this subject to be met with in chemical literature since the time of its promulgation. Although the author takes hydrogen as the first stage in his evolutional series he admits, with Prout, that this element "may have been evolved from an ethereal substance of much greater tenuity." Under the seven stages of condensation comprised from Hn to H7n. The author arranges all the chemical elements in a tabular form, leaving gaps for unknown elements, and correcting the atomic weights where necessary so as to make them accord with the hypothesis. Some of the results of this treatment have already been alluded to. The way in which Bode's method is applied will be understood by taking one example, viz. the first series, Hn:—

$$
\begin{array}{rcrcrcl}
0 & & 0 & & 7 & = \mathrm{Li} = & 7 \\
1 & \times & 23 & - & 0 & = \mathrm{Na} = & 23 \\
2 & \times & 23 & - & 7 & = \mathrm{K} = & 39 \\
3 & \times & 23 & - & 7 & = \mathrm{Cu} = & 62 \\
4 & \times & 23 & - & 7 & = \mathrm{Rb} = & 85 \\
5 & \times & 23 & - & 7 & = \mathrm{Ag} = & 108 \\
6 & \times & 23 & - & 7 & = \mathrm{Cs} = & 131 \\
7 & \times & 23 & - & 7 & = \text{—} = & 154 \\
8 & \times & 23 & - & 7 & = \text{—} = & 177 \\
9 & \times & 23 & - & 7 & = \mathrm{Hg} = & 200
\end{array}
$$

The rule of construction is: multiply the atomic weight of the second member (Na=23 in the above) by the arithmetical series and subtract the atomic weight of the first member (Li = 7 in the above) from the products; the results are the atomic weights of the elements of the series. This method is applied also to the group H2n with tolerable success, *provided the atomic weights are modified to even numbers and that the atomic weight of beryllium is made* 8. Mr. Wilde's second group is given below:—

Be, 8; Mg, 24; Ca, 40; Zn, 64; Sr, 88; Cd, 112; Ba, 136; *x*, 160; *x*, 184; Pb, 208.

This is presumably one of the new relations between the atomic weights referred to in the title of the paper. In the third group, however (H3n), very considerable modifications of the atomic weights have to be made, as will be seen from the author's results:—

C, 12; Al, 27; Sc, 42*; Ce, 69*; Ga, 96*; Y, 123*; In, 150*; Er, 177*; Tl, 204; Th, 231.

The six numbers marked with an asterisk stand for 44; 92 or 141; 70; 61·7 or 89·5; 75·6 or 113·4; and 170·6 respectively. A system which necessitates this amount of manipulation of experimental results will certainly fail to commend itself for adoption by chemists. The proposed change of beryllium from 9·2 to 8 is directly opposed by the determination of the vapour density of the chloride by Nilson and Pettersson, and if adopted would cause this element to become still more divergent from the law of Dulong and Petit. The vapour density of indium chloride as determined by Nilson and Pettersson is in accordance with the accepted atomic weight of that element and opposed to that given by Mr. Wilde. The elements associated in the first and second groups respectively, may be allowed to pass as natural allies, but the separation of carbon from its analogues, silicon, titanium, &c., and its association in the third group with aluminium, scandium, gallium, &c., is a violation of known relationships. The four halogens according to their atomic weights belong to the author's first (Hn) group. They are regarded as the negative analogues of the alkaline metals and are therefore placed in a separate column in such a way as to bring out the relation that there is a constant difference of 4 between each halogen and its positive analogue :—

Na, 23 - 4 = 19, F ; K, 39 - 4 = 35, Cl ; Rb, 85 - 4 = 81 Br ; Cs, 131 - 4 = 127, I.

In a similar way the oxygen group is made into a negative column having positive analogues in the H2n group and showing a constant difference of 8 :—

Mg, 24 - 8 = 16, O ; Ca, 40 - 8 = 32, S ; Sr, 88 - 8 = 80, Se ; Ba, 136 - 8 = 128, Ta.

Of course chemists have long been familiar with various numerical relationships between groups of allied elements, but this does not appear as sufficient evidence for altering the atomic weights of Br, Cl and Se, unless these relationships can be conclusively shown to be the necessary result of a general law.

But apart from such defects as have been pointed out, it will be seen that the proposed grouping breaks down altogether after the third group. The author is hardly fair when he says (Preface, p. iv.) :—"While the multiple relations subsisting among the atomic weights of the other series of elements are highly interesting, they do not possess, in the present state of our knowledge, that degree of precision which is the distinguishing feature of the series Hn and H2n. An exception might, however, be made in favour of the series H3n, &c."

As a matter of fact it is not a question of "degree of precision" at all, for, as far as we can see, the other groups do not lend themselves to the Bodeian method ; at any rate, not in the form applied to the groups Hn, H2n, and H3n. We give the author's results as compared with those obtained by the application of his own method :—

Group H4n.

$x = 16$; $x = 32$; Ti = 48 ; Ge = 72 ; Zr = 92 ; Sn = 116 ; La = 140 ; $x = 164$; D = 188 ; U = 240.

The numbers obtained by the rule (1, 2, 3, 4, &c. × 32, and 16 subtracted from each product) are 32, 48, 80, 112, 144, 176, 208, 240, &c., which, after Titanium, do not represent any atomic weights in the group till Uranium is reached.

Group H5n.

B = 10 ; P = 30 ; V = 50 ; As = 75 ; Nb = 95 ; Sb = 120 ; $x = 140$; $x = 165$; Ta = 185 ; Bi = 210.

Calculated (1, 2, 3, 4, &c. × 30 and 10 subtracted from each product) the numbers are :—30, 50, 80, 110, 140, 170, &c.

Group H6n.

$x = 18$; $x = 36$; Cr = 54 ; Mo = 96 ; $x = 144$; W = 186.

Calculated (1, 2, 3, 4, &c. × 36 and 18 subtracted from each product) the numbers are :—36, 54, 90, 198, &c.

Group H7n.

N = 14 ; Si = 35 ; [Fe = 56 ; Mn = 56 ; Ni = 56 ; Co = 56] ; [Pd = 105 ; Rh = 105 ; Ru = 105 ; Da = 105] ; [Au = 196 ; Pt = 196 ; Ir = 196 ; Os = 196].

Calculated (1, 2, 3, 4, &c., × 35 and 14 subtracted from each product) the results are :—35, 56, 91, 126, 161, and 196.

The association of nitrogen with silicon and the metals of the iron and platinum groups is, to say the least of it, incomprehensible. We have thought it desirable to give this analysis, for no reason is given in the paper for this particular grouping after the third series, beyond the well-known chemical relationships of the elements which, as we have seen, is sometimes violated in a most unaccountable way. The groups are obviously not constructed by the Bodeian method ; the atomic weights are modified in many cases by one or two units, and the result is a classification which differs only from the received classification on points which cannot possibly be conceded by chemists. The reason why silicon is separated from its analogues is as follows :—

> Now, if silicon were the true analogue of titanium, the oxides of these elements should be isomorphous, whereas the crystalline form of quartz is hexagonal, while rutile, anatase, brookite, zirconia, and tinstone (similar oxides of members of the series H4n) are tetragonal ; consequently, silicon does not belong to the series H4n.

This is a point, and out of justice to the author we give it for what it is worth,[1] but the atomic weight of silicon has been determined by the vapour density of its chloride, and the result is fatal to Mr. Wilde's classification. His attempt to justify the atomic weight 35 by an appeal to the specific heat is unfortunate, because he takes the old determination by Regnault (0·176) instead of the more recent determination by Weber (0·203 at 230° C.). Moreover, he is inconsistent in not allowing the same correction for boron and the other elements which deviate from Dulong and Petit's law.

We cannot go much further into the details of this paper. Enough has been written to justify the disappointment which we expressed at the outset, and it is only the intrinsic importance of all questions bearing upon the origin of the elements that has warranted such extended treatment. It appears that the numerical relations which are brought out by the author's method have either long been known or else—as in his application of the Bodeian method—they do not exist beyond a limited number of groups. The results do not take us beyond the point at which chemists were left by Döbereiner, Pettenkofer, Dumas, and numerous other chemists who, for three-quarters of a century, have directed attention to such numerical relationships. In some respects—such, for example, as in the exactness with which the atomic weight of an element is the arithmetical mean of the elements above and below it in the same series—Mr. Wilde's numbers express the relationship more closely than those of any other author ; but this agreement is simply brought about by forcing the atomic weights into the requirements of the case. The increase in density as the odd and even series are ascended, is nothing more than an imperfect way of stating the well-known relationship between atomic weight and atomic volume, which is so much better shown by Lothar Meyer's curves. The table of elements presented by Mr. Wilde ignores that fundamental principle of periodicity or recurrence of properties which is the key-note of Mendeléeff's system, and which has led to the general adoption of that system by chemists. We do not pretend that Mendeléeff's classification is faultless ; the illustrious founder of the Periodic Law would be the first to admit that his system has certain imperfections. Mr. Wilde has emphasized a few of these in his preface, and he somewhat summarily dismisses the whole scheme in the following words :—

> From the numerous discrepancies which present themselves in the classification of the elements when arranged in the regular order of their atomic weights, it will be obvious that the idea of recurring properties or periodic functions, in terms of the vertical series of Newlands or the horizontal series of Mendeleeff, has no more relation to chemical science than the law of the increase of population, or the laws of variation and inheritance in organic species.

This paragraph, penned in the present year, will, perhaps better than any other statement that could be reproduced from the paper, enable chemists to form a correct estimate of the value of the work and of the author's qualifications for dealing with the question of the origin of the chemical elements.

R. MELDOLA.

---

[1] "Stannic and titanic oxides resemble silica both physically and chemically . . . . . they might be expected to form analogous compounds, and be isomorphous with silica, as Marignac (1859) found actually to be the case." Mendeléeff's "Principles of Chemistry," vol. ii. p. 95.